

geologische Reichsanstalt; and under these circumstances Prof. Zittel of Munich has pointed it out to the author of the present paper as a promising field of study. Mr. Dale's work will certainly be of considerable use to future explorers of the district, though not carried out in sufficient detail to warrant, in his own case, any very important generalisations. Indeed, the memoir consists almost wholly of transcriptions of notes and rough drawings of sections relating to a number of different localities which are indicated by reference to a key-map. The author's general conclusions, so far as they go, are shown in a very clear and useful table, from which it appears that at this point of the Alps, the Jurassic and Rhætic strata (including in the former the Tithonian) have a united thickness of from 6,000 to 7,000 feet. Vast as is the estimate, no one acquainted with this or the surrounding districts will be inclined to regard it as excessive.

Mr. Dale has evidently made good use of his opportunities, so far as they have gone, and has given us in this memoir the results of a piece of well-directed observation. We hope to have further details from his pen concerning the same interesting region. The list of *errata*, which is rather long for a memoir of the proportions of the present, does not by any means exhaust the whole of the printer's errors. We are tempted to fear that Mr. Dale is not sufficiently careful in keeping so distinct from one another, as behoves a working geologist, his notes relating to various subjects; for, by some strange chance a stray page of a sermon seems to have fallen into the hands of the compositor and to have been set up by him at the end of the author's geological notes.

J. W. J.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

"Geographical Distribution of Animals"

I FIND that Mr. Wallace in his new work on the "Geographical Distribution of Animals" when stating the limits of his Ceylon sub-region (vol. i. p. 327), gives among mammals the genus *Tupaia* and among birds "a species of *Myiophonus*, whose nearest ally is in Java," as characteristic of that sub-region. Further, in the tabular statement (vol. ii. p. 187), *Tupaia* is altogether omitted from the Indian sub-region.

It is not my intention to enter here into the general question of the divisions of the oriental region which Mr. Wallace has adopted. The subject has I know been undertaken by at least one well-known Indian naturalist. My object at present is simply to record the fact that I have found both *Tupaia Ellioti* and *Myiophonus Horsfieldi* ranging together far to the north of the limits given by Mr. Wallace for his Ceylon sub-region.

Tupaia I first met with at an elevation of about 1,500 feet in the Sutpuri Hills, near the Pachmari plateau in the Central Provinces (P.I.A.S.B., April, 1874), lat. 22° 20'. Subsequently I found it in Sambalpur, which is the most eastern district of the Central Provinces (lat. 21° 30'). But the former does not even give its extreme northern limit as it has been found in the Kurrucepur hills of the Monghyr district (lat. 25°).

Myiophonus Horsfieldi I first shot in Sirsugra—a native state in Western Bengal (lat. 23°). Afterwards in the Sutpurs, where it occurred with *Tupaia* as above, and finally I obtained it also in Sambalpur, where it was found at elevations under 1,000 feet above the sea. Still further north it has been obtained at Mount Aboo ("Stray Feathers," vol. iii. p. 469), lat. 25°.

Myiophonus is, it is true, included in Mr. Wallace's list of Oriental genera in Central India, but its special employment as a characteristic form of the Ceylon sub-region seems scarcely compatible with a knowledge of its now ascertained wide range through continental India.

During the ensuing field season I expect to be engaged in the geological examination of one of the widest and least known parts of India—the area between the Godavari and Mahanudi Rivers. I have great hopes of discovering there further facts regarding the range of species whose limits are now only imperfectly known. In the meantime I may state that during the

present year I have shot *Harpactes fasciatus* in Sambalpur, thus confirming the late Col. Tukell's statement of its occurrence in the same general tract of country. The above allusion to *Tupaia* leads me on to record here that I have met with two other species of the genus.

During an ornithological tour which was made in 1873 by a party of which I was a member—through the islands of the Andaman and Nicobar groups—we obtained a species of *Tupaia* on the Island of Preparis. Our specimen appears to be identical with *T. Pequensis*, which occurs from Pegu to Sikkim. On the Great Nicobar we shot a specimen of the species described in the Novara account as *Nicobariensis*, and considered then to be worthy of generic distinction.

In Preparis, it may be added, we also shot a small grey squirrel which is allied to if not identical with *S. Assamensis*. These, with a monkey (*M. carbonarius*?), pigs, and probably rats and bats, constituted so far as we could ascertain the mammal fauna of the island.

Preparis I should perhaps explain is the most northern of the Andaman group lying between Cape Negrais and the Cocos.

Mr. Wallace has I observe included the Nicobar Islands in the Malayan sub-region and the Andamans in the Indo-Chinese. This separation of the two groups is, I believe, fully justified by the facts.

Some years ago when working at the avifauna of these islands (J.A.S.B., 1872, p. 274), while recognising the fact of a number of species being common to both groups, I could not resist a conviction as to the existence of a strong Malayan stamp upon the birds which are peculiar to the Nicobars.

In conclusion Mr. Wallace's magnificent work needs no praise from me; but as a field worker and observer I may perhaps venture here to offer my thanks for the valuable mine of information which it affords.

V. BALL,
Calcutta, September 28 Geological Survey of India

European Polygalas

IN view of a monograph of the order Polygalaceæ which I have in preparation, may I make use of your columns to say that I should be greatly obliged to any correspondents who can send me specimens of any of the less common European *Polygalas*, especially *P. exilis*, *monspeliaca*, *microphylla*, *saxatilis*, *Preslia*, *nicaeensis*, *flavescens*, *rosea*, *sibirica*, *supina*, *venulosa*, *anatolica*, or any well-marked varieties. I shall be glad to offer in exchange specimens of some of the rarer British plants.

6, Park Village, East, Regent's PARK, London, October 28 ALFRED W. BENNETT

The Solidity of the Earth

IN his opening address to the Mathematical and Physical Section of the British Association, Sir William Thomson affirmed "with almost perfect certainty, that, whatever may be the relative densities of rock, solid and melted, or at about the temperature of liquefaction, it is, I think, quite certain that cold solid rock is denser than hot melted rock; and no possible degree of rigidity in the crust could prevent it from breaking in pieces and sinking wholly below the liquid lava," and that "this process must go on until the sunk portions of the crust build up from the bottom a sufficiently close-ribbed skeleton or frame, to allow fresh incrustations to remain bridging across the now small areas of lava, pools, or lakes" (NATURE, vol. xiv. p. 429).

This would doubtless be the case if the material of the earth were chemically homogeneous or of equal specific gravity throughout, and if it were chemically inert in reference to its superficial or atmospheric surroundings. But such is not the case. All we know of the earth shows that it is composed of materials of varying specific gravities, and that the range of this variation exceeds that which is due to the difference between the theoretical internal heat of the earth and its actual surface temperature.

We know by direct experiment that these materials, when fused together, arrange themselves according to their specific gravities, with the slight modification due to their mutual diffusibilities. If we take a mixture of the solid elements of which the earth, so far as we know it, is composed, fuse them, and leave them exposed to atmospheric action, what will occur?

The heavy metals will sink, the heaviest to the bottom, the lighter metals (i.e. those we call the metals of the earths, because they form the basis of the earth's crust) will rise along with the silicon, &c., to the surface; these and the silicon will oxidise and combine, forming silicates, and with a sufficient supply of

carbonic acid, some of them, such as calcium, magnesium, &c., will form carbonates when the temperature sinks below that of the dissociation of such compounds. The scoria thus formed will float upon the heavy metals below and protect them from cooling by resisting their radiation; but if in the course of contraction of this crust, some fissures are formed reaching to the melted metals below, the pressure of the floating solid will inject the fluid metal upwards into these fissures to a height corresponding to the floatation depth of the solid, and thus form metallic veins permeating the lower strata of the crust. I need scarcely add that this would rudely but fairly represent what we know of the earth.

But it may be objected that I only describe an imaginary experiment. This is true as regards the whole of the materials united in a single fusion. Nobody has yet produced so complete a model with platinum and gold in the centre, and all the other metals arranged in theoretical order, and with the oxidised, silicated, and carbonated crust outside; but with a limited number of elements this has been done, is being done daily, on a scale of sufficient magnitude amply to refute Sir William Thomson's description of a fused earth solidifying from the centre outwards.

This refutation is to be seen in our blast furnaces, refining furnaces, puddling furnaces, Bessemer ladles, steel melting pots, cupels, foundry crucibles; in fact, in almost every metallurgical operation down to the simple fusion of lead or solder in a plumber's ladle, with its familiar floating crust of dross or oxide.

As an example I will—on account of its simplicity—take the open hearth finery, and the refining of pig iron. Here a metallic mixture of iron, silicon, carbon, sulphur, &c., is simply fused and exposed to the superficial action of atmospheric air. What is the result?

Oxidation of the more oxidisable constituents takes place, and these oxides at once arrange themselves according to their specific gravities. The oxidised carbon forms atmospheric matter and rises above all as carbonic acid, then the oxidised silicon being lighter than the iron floats above that, and combines with any aluminium or calcium that may have been in the pig, and with some of the iron; thus forming a siliceous crust closely resembling the predominating material of the earth's crust. The cinder of the blast furnace, which in like manner floats on the top of the melted pig iron, resembles still more closely the prevailing rock-matter of the earth, on account of the larger proportion, and the varied compounds of earth-metals it contains.

When the oxidation in the finery is carried far enough, the melted material is tapped out into a rectangular basin or mould, usually about 10 feet long and about 3 feet wide, where it settles and cools. During this cooling the silica and silicates—*i.e.*, the rock-matter—separate from the metallic matter and solidify on the surface as a thin crust, which behaves in a very interesting and instructive manner. At first a mere skin is formed. This gradually thickens, and as it thickens and cools becomes corrugated into mountain chains and valleys much higher and deeper, in proportion to the whole mass, than the mountain chains and valleys of our planet. After this crust has thickened to a certain extent volcanic action commences. Rifts, dykes, and faults are formed by the shrinkage of the metal below and streams of lava are ejected. Here and there these lava streams accumulate around their vent and form isolated conical volcanic mountains with decided craters, from which the eruption continues for some time. These volcanoes are relatively far higher than Chimborazo. The magnitude of these actions varies with the quality of the pig-iron.

The open hearth finery is now but little used, but probably some are to be seen at work occasionally in the neighbourhood of Glasgow, and I am sure that Sir William Thomson will find a visit to one of them very interesting. Failing this he may easily make an experiment by tapping into a good-sized "cinder bogie," some melted pig-iron from a puddling furnace (taking it just before the iron "comes to nature"), and leaving the melted mixture to cool slowly and undisturbed.

For the volcanic phenomena alone he need simply watch what occurs when in the ordinary course of puddling the cinder is run into a large bogie and the bogie is left to cool standing upright. I need scarcely add that these phenomena strikingly illustrate and confirm Mr. Mallett's theory of earthquakes, volcanoes, and mountain formation.

In merely passing through an iron-making district one may see the results of what I have called the volcanic action, by simply observing the form of those oyster-shaped or cubical blocks of cinder that are heaped in the vicinity of every blast

furnace that has been at work for any time. Radial ridges or consolidated miniature lava streams are visible on the exposed face of nearly, if not quite all, of these. They were ejected or squeezed up from below while the mass was cooling, when the outer crust had consolidated but the inner portion still remained liquid. Many of these are large enough and sufficiently well marked, to be visible from a railway carriage passing a cinder heap near the road.

I intended to have made a few remarks upon another of Sir William Thomson's arguments for the earth's solidity, but the pressure of necessary business compels me to postpone them.

W. MATTIEU WILLIAMS

Belmont, Twickenham, October 17

Are We Drying Up?

IN NATURE, vol. xiv. p. 527, there is an article condensed from one by Prof. Whitney, with the alarming title "Are We Drying Up?" with a number of facts to prove that we are—that in the temperate zone at least, the supply of water in the rivers and lakes is failing at a more rapid rate than the destruction of forests will account for.

Supposing this to be true, it can have only one of two causes: a decrease in the area of the ocean, from which the rivers are supplied through evaporation and rainfall; or a diminution in the supply of heat from the sun, which would of course diminish evaporation.

It is scarcely necessary to say that any perceptible decrease in the area of the ocean during historical time is theoretically most improbable, and that practically there is no evidence of it.

A diminution in the radiation of heat from the sun is not impossible: and it is also shown in the concluding paragraph of your article, that a diminution in the obliquity of the ecliptic, which I believe is now going on, must tend to diminish the supply of solar heat to the higher latitudes, and, consequently, to diminish evaporation and rainfall there.

But were it true the supply of heat to the temperate zone were sensibly diminishing from either of these two causes, the fall of temperature would be quite as noticeable as the diminution of rainfall; and we should have proof of this from historical evidence as to the distribution of cultivated and wild plants. But there is no general evidence of the kind. In Iceland and Siberia, it is true, there appears to be some evidence of the summers having become colder, but in the more temperate regions the range of cultivated plants seems to have remained unchanged, or at least not to have receded, from the earliest periods of which we have any record.

It appears, therefore, most likely that the diminution of rainfall, where it really exists, is a merely local phenomenon.

It seems to be forgotten, that while any local diminution of rainfall is certain to give proof of itself in unfilled water-courses, any corresponding increase of rainfall in another locality will not prove itself in any equally visible way.

There appears to be little doubt of the recent desiccation of the region round the Caspian and Aral Seas, but it admits of being explained by a local cause. The Caspian has, within the last few thousand years, been cut off by a geological change from the Black Sea or the Arctic Ocean, or from both; since then it has shrunk in consequence of the excess of evaporation over rainfall, and the more its area has diminished, the less is the rainfall of the surrounding regions.

Some of the facts quoted, showing that rivers are ceasing to be navigable, do not necessarily prove that the rainfall is less than formerly, but only that the flow of the rivers is less regular. It appears certain that the destruction of forests, and the introduction of agricultural drainage tend to this result, by throwing the water more rapidly off the land. But there is also a good deal of evidence to show that the destruction of forests, and of vegetation generally, tends to diminish rainfall. The most satisfactory instances, in every sense, are those of the converse kind, which show that rainfall may be increased by the judicious fostering of vegetation. I will mention a few of these which occur to me, without being able to remember my authorities.

In Lower Egypt, rain has become much commoner since the formation of extensive date-tree plantations; and the flow of water in the Kedron, in the neighbourhood of Jerusalem, has become more abundant and regular since the planting of groves of mulberry and other trees about its sources. In the arid volcanic Island of Ascension, where trees would not have lived, showers of rain have been attracted, by planting such herbaceous plants as are best able to endure the almost permanent drought. The